Subject to Protective Order

EXPERT WITNESS REPORT

La Union del Pueblo Entero v. Abbott, No. 5:21-cv-844 (W.D. Tex.) (lead case)

Submitted by

Mark Hoekstra, PhD

Date of Report

April 5, 2023

I. Introduction

1. I have been engaged to respond to the second supplemental report dated March 6 2023, that was written by Professor Kenneth R. Mayer in the consolidated case *La Union del Pueblo Entero v. Abbott*, No. 5:21-cv-844 (W.D. Tex.). My analysis is based on my knowledge and experience as an active research economist who is well versed in the frontier empirical methods used in causal analyses.

II. Qualifications and Compensation

2. I am the Rex B. Grey Professor of Economics at Texas A&M University in College Station, Texas, where I have been on the faculty since 2011. Prior to arriving at Texas A&M, I was an assistant professor of economics at the University of Pittsburgh. I received my PhD in Economics from the University of Florida in 2006. I have published more than 20 papers in peer-reviewed journals in economics, including the American Economic Association's (AEA) top journal of *American Economic Review* as well as the top AEA journals relevant to my field (*American Economic Journal: Applied Economics* and *American Economic Journal: Economic Policy*) and top field journals (*Journal of Labor Economics; Journal of Public Economics*; *Journal of Human Resources*). These studies have used a wide variety of administrative datasets, including data on voting. I serve as a reviewer for approximately 20 paper submissions per year, including for top economics journals. I serve as an Associate Editor at the *Journal of Labor Economics* (since 2018) and at the J*ournal of Human Resources* (since 2015), which are the top two field journals in labor economics.

3. The common theme throughout both my research and my teaching is careful attention to the assumptions underlying various research designs used to assess the causal impact of policies. Some of my research is used as examples in the textbook *Causal Inference: The Mixtape* by Scott Cunningham, which is a leading graduate-level book on empirical methods used in economics. I teach

a PhD-level field course in labor economics, the focus of which is on understanding and assessing the various research methodologies used by economists and other social scientists. These include the methods used to test for racial and gender bias in different settings. I also teach part of the core 1st-year PhD sequence in econometrics, in which I focus on how to assess causality in non-experimental contexts. I am perhaps best known among the PhD students as someone who offers advice on research projects, including whether the proposed method is sufficient to answer the question at hand. I recently won department-level awards for both the quality of graduate teaching and the quality of feedback given to students. During my time at Texas A&M, I have chaired more than 10 dissertation committees, and many of my students have gone on to careers at R-1 research universities. I have served on the committees of countless more PhD students.

4. I also hold an appointment as a Research Fellow at the National Bureau of Economic Research based in Cambridge, Massachusetts, and as a Research Fellow at the Institute for Labor Economics (IZA) based in Bonn, Germany. In 2012 I received the IZA Young Labor Economist Award.

5. I am being compensated for my time at the rate of $600/hour. I have not previously testified as an expert witness.

III. Assessment of Professor Mayer's March 6 supplemental report – Overview

6. Did the new mail-in voting requirements of SB 1 cause a sufficiently large burden on Texas voters to actually reduce voting? And if so, to what extent did any such burden disproportionately impact minority voters? Professor Mayer's March 6 report asserts that in the November 2022 general election in Dallas, Harris, and Hidalgo counties, 2,949 voters, out of a total of 1,881,445 voters, were "disenfranchised" as a result of the new mail-in voting rules implemented by SB 1. Professor Mayer also asserted that the "disproportionate impact that these provisions have

on African American and Hispanic voters is material and statistically significant." Finally, Professor Mayer asserted that the new mail-in voting rules do not contribute to election security or the integrity of the voting process.

7. If true, these assertions would certainly be troubling. However, I show below that these assertions are at best unfounded, and at worst false. In particular, they are based on incorrect analysis and incorrect interpretation.

8. I show the following:

A. Professor Mayer repeatedly misinterprets ballots not counted due to the identification requirements as "disenfranchised voters". In fact, an alternative interpretation equally consistent with the facts is that these 2,949 votes in question were illegitimate. There is nothing in Professor Mayer's report, or in the underlying data, to indicate whether or not those rejected mail-in votes were cast legally. As a result, even if these votes went uncounted because SB 1 reduced illegitimate votes – which was the stated goal of the legislation – Professor Mayer would misinterpret that as evidence of disenfranchisement.

B. Professor Mayer's assertion that mail-in ballot rejections increased as a consequence of SB 1 is based on pre-SB 1 rejection data that appear to be incorrect. In my report, I highlight several ex ante reasons to doubt the quality of the pre-SB 1 rejection data, relative to the post-SB 1 rejection data.

   Moreover, these concerns are borne out in the data. I demonstrate using Professor Mayer's data that mail-in ballot rejection rates *for reasons other than SB 1* more than doubled from before SB 1 to after SB 1. These rejections cannot, by definition, be caused by SB 1. This suggests that poor record-keeping of rejections prior to SB 1, or some other factor, is driving some or all of the increase in rejection rates observed. This casts serious doubt on the credibility of pre- versus post-SB 1 rejection rate comparisons.

C. In asserting that SB 1 does not contribute to election security, Professor Mayer repeatedly, and improperly, assumes that absence of evidence is evidence of absence. Much if not all of what Professor Mayer emphasizes is the number of election fraud cases that are prosecuted, which is small relative to total votes cast. However, I show that even for types of criminal misconduct such as property crime, fewer than 7 percent of incidents are prosecuted. It would be wrong to assume those other incidents never happened. That low prosecution rate is striking, given that property crimes are almost certainly detected by the victim, the victim is typically incentivized to report it, and there is often evidence to support a prosecution. By comparison, election fraud is much less likely to be detected and thus reported by either the victim or election personnel, and is likely more difficult to prove. Given that, would we not expect the prosecution rate of election fraud to be a small fraction of that of property crime? As a result, the prudent view is that it is difficult or even impossible for anyone to know how often mail-in ballot fraud occurs.

In addition, Professor Mayer ignores the indisputable public perception that election fraud is widespread. As a result, even if one believes there is zero election fraud, one can also believe there are benefits associated with enacting safeguards such as SB 1 to help convince the electorate of the same.

D. Professor Mayer's disparate impact analysis is characterized by three critical flaws.

i. Professor Mayer computes disparate impact incorrectly. While Professor Mayer accounts for differential rates of ballot rejections among voters who chose to vote absentee, he does *not* account for differences across groups in the likelihood of voting absentee. This is incorrect; I show doing so results in nonsensical conclusions. Professor Mayer's incorrect approach for estimating disparate impact is also inconsistent with a (correct) statement Professor Mayer made in his own report about how to evaluate disparate impact. Similarly, it is also inconsistent with a (correct) statement about how to evaluate disparate impact made by another expert for the plaintiff, Professor Eric McDaniel.

a. While I presume this was an honest mistake by Professor Mayer, the practical implications are important. I show correcting this mistake in Harris County indicates that SB 1's rules generate disparate impact *against* voters predicted to be White, in favor of voters predicted to be Hispanic. This is the exact opposite of what Professor Mayer's incorrect analysis suggested. Similarly, while Professor Mayer's findings assert evidence of disparate impact against predicted-Asians in favor of those predicted to be White, the correct analysis shows there is no difference between the two groups. Additionally, while I do not have access to the necessary data for Dallas County to correct his analysis, it is clear the problem is sufficient as to cast serious doubt on, if not entirely discredit, the results in Professor Mayer's report.

ii. Professor Mayer's disparate impact analyses do not use data on the actual race and ethnicity of voters. Rather, it is inferred using data on surname and residential neighborhood.

a. Unsurprisingly, the creators of the prediction algorithm document that the predictions come with significant error. This has important implications for any analysis across racial or ethnic groups. Yet Professor Mayer's analysis seemingly ignores these errors. For example, Professor Mayer's analysis predicts there are 187,416 votes cast by Black voters. In contrast, I demonstrate that if we take into account the classification error documented by the scholars who developed the method used by Professor Mayer, there were actually an estimated 342,076 Black voters. Given the sheer number of misclassified Black voters in the analysis, it should come as no surprise that a wide range of estimates of racial differences are possible, depending on the assumptions made.

b. Indeed, I show that if one accounts for the classification errors, estimates of the rejection rates of actual Black voters in Harris County range from 0 (and thus substantially lower than Whites), to rates that are substantially higher than Whites. Put differently, Professor Mayer's interpretation depends crucially on assumptions he implicitly made, but did not acknowledge, about the impact of the classification error on the analysis. As a result, a more prudent interpretation is that because classification

error is so large, it is hard to say anything with a high degree of certainty about differences in voting across actual racial and ethnic groups.

iii. Professor Mayer does not acknowledge the importance of voter substitution across voting methods. Yet this is a critical feature to assessing any potential burden imposed by SB 1's safeguards on mail-in voting. Even the strictest restriction on mail-in voting will not burden voters if they consider another method of voting as a perfect substitute. In fact, the best empirical evidence on this question indicates that Texan voters view in-person voting as a close-if-not-perfect substitute for mail-in voting. In short, it shows that restrictions that are much stronger than those imposed by SB 1 only change *how* Texans vote, not *whether* they vote.

Yet Professor Mayer's report only references this possibility once, in a footnote about a small set of voters. Beyond that, it is unclear whether Professor Mayer considered the likelihood that legitimate voters whose mail-in ballots were rejected would vote in person. It is also unclear if he is including voters who subsequently cured their mail-in ballot or voted in person after a rejection in his final number of 2,949 ballots rejected due to SB 1. Regardless, another expert for the plaintiff, Professor Eitan Hersh, indicated in his February 3, 2023 report that across all of Texas, there were only 6,355 votes, out of 8.1 million cast in the 2022 general election, that were rejected due to SB 1's requirements and where a vote by the same individual was not subsequently cast and counted. And there is nothing in Professor Hersh's report, or Professor Mayer's report, to suggest that these votes were cast legitimately rather than illegitimately. There is also nothing to suggest that these votes were not rejected due to voter error in completing the ballot, which means even this small number may decline as voters become more familiar with the new process.

E. To what extent were drive-thru voters from the 2020 election in Calhoun County unwilling, or unable, to vote in subsequent elections, when drive-thru voting was no longer permissible? Professor Mayer would have you believe that "the elimination of drive-thru voting made it more difficult for Hispanic voters, in particular, to cast a vote in the 2022 general election".

The fact is, however, that there is no evidence that the individuals who voted in 2020 using drive-thru voting were negatively impacted *at all* by the elimination of drive-thru voting. Instead, I show that those voters participated in the 2022 primary and general elections at higher rates than did other Calhoun County residents who also voted early in the 2020 election. This is true both in the aggregate, and for voters predicted to be White, Non-White (i.e., all other groups combined), and Hispanic. For example, while 29.8 percent of drive-thru voters predicted to be Hispanic voted in the 2022 primary, only 18.8 percent of predicted-Hispanic non-drive-thru-early-voters did so. Similarly, 62.3 percent of predicted-Hispanic 2020 drive-thru voters subsequently voted in the 2022 general election, compared to only 47.9 percent of their non-drive-thru-voting predicted-Hispanic counterparts.

As a result, it is incorrect for Professor Mayer to imply that predicted-Hispanic drive-thru voters were disparately impacted. Rather, it is clear they were more willing and able than their non-drive-thru-voting counterparts to subsequently cast votes in 2022.

In addition, I show that the statistical test employed by Professor Mayer is flawed. I show that if the same test is applied to non-drive-thru early voters from 2020, that test would also

conclude evidence of disparate impact. That is clearly untenable, since those voters did not even use the drive-thru voting option in 2020.

Moreover, both of these criticisms leave aside the issue that as referenced earlier, Professor Mayer does not observe the race or ethnicity of voters. Rather, he only observes a prediction of race and ethnicity, which comes with a considerable amount of error. Accounting for the uncertainty in classifying ethnicity casts further doubt on the credibility of these findings.

IV. Professor Mayer's (Mis)Interpretation of Mail-In Ballot Rejections, the Effect of SB 1 on Mail-In Ballot Rejections, and the Benefits of SB 1 to Election Security

A. Professor Mayer's (Mis)Interpretation of Mail-In Ballot Rejections

9. Professor Mayer's analysis of mail-in voting examines Dallas, Harris, and Hidalgo counties. Table 1 replicates these results. It shows that out of the 1.9 million votes cast and counted in those three counties in the 2022 general election, there were 2,949 mail-in ballots rejected (0.16 percent of total ballots) for reasons relating to SB 1's identification requirements.

10. Professor Mayer chooses to interpret these ballot rejections as evidence that SB 1 "disenfranchised" legitimate voters. The trouble is that there is nothing in Professor Mayer's report, or in the underlying data, to indicate whether these 2,949 votes were legally cast, rather than illegitimately cast. Neither Professor Mayer, nor I, can distinguish between these two interpretations of the facts that Professor Mayer presents.

11. This is particularly concerning given that the stated purpose of SB 1 was to reduce the likelihood of fraudulent votes being cast and counted. Under Professor Mayer's chosen interpretation, any fraudulent votes rejected under SB 1 would be interpreted as evidence of disenfranchisement. This is clearly incorrect.

Table 1: Rejection Rates of Ballots Due to SB 1, Per Professor Mayer's Report

County	Total Votes Cast in 2022 General Election	2022 Total Mail Absentee Ballots Rejected on Account of SB 1, per Mayer Table 1	Fraction of Total votes cast rejected due to SB 1	% Total votes cast rejected due to SB 1
	(1)	(2)	(3) = (2)/(1)	(4) = (2)/(1)*100
Dallas	626,944	320	0.000510	0.0510%
Harris	1,110,726	2,558	0.002303	0.2303%
Hidalgo	143,775	71	0.000494	0.0494%
Totals	1,881,445	2,949	0.001567	0.1567%

Notes: In Column (1), 2022 total votes cast was computed as the number of cumulative in-person votes reported on the Secretary of State Website at https://earlyvoting.texas-election.com/Elections/getElectionEVDates.do at the close of business on election day in 2022, plus the number of mail absentee ballots returned reported in Column 1 of Table 1 from Mayer. Column (2) is taken directly from Table 1 in Mayer.

B. Professor Mayer's assertion that SB 1 caused an increase in mail ballot rejection rates is based on pre-SB 1 data that appear to be incorrect

12. Professor Mayer also asserts that SB 1 caused an increase in mail-in ballot rejection rates. The trouble with this assertion is that any pre- versus post-policy comparison, including this one, is only as good as the underlying data. Unfortunately, there are reasons to doubt the quality of ballot rejection data prior to SB 1. That is in part because another bill, passed around the same time as SB 1, introduced a ballot tracker. In addition, SB 1 introduced a cure process. For these reasons, counties were required to collect more data after SB 1 compared to before. This creates problems for before-versus-after comparisons.

13. These concerns are borne out in the data. Columns (2) and (3) of Table 2 show the number and rate of mail-in ballot rejections in the 2022 general election for reasons *other than the identification requirements associated with SB 1*. It shows that 1.66, 0.86, and 0.67 percent of mail ballots in Dallas, Harris, and Hidalgo counties were rejected. Columns (4) and (5) show that ballot rejections for non-SB 1 reasons were 2.7 to 2.8 times higher in Dallas County and Harris County in 2022, compared to all ballot rejections for 2020. Yet there is no reason to expect that SB 1 should increase the rate of ballot rejections for reasons that have nothing to do with SB 1's identification requirements.

Rather, this result indicates something is clearly wrong with the underlying data, the comparison, or both. At best, this casts serious doubt on the reliability of before-versus-after comparisons in assessing the causal impact of SB 1 on ballot rejection rates.

Table 2 Rejection Rates of Mail-In Ballots for Non-SB 1 Reasons, 2020 and 2022

County	2022 Mail Absentee Ballots Returned	2022 Total Mail Absentee Ballots Rejected for Reasons NOT Associated with SB 1	2022 Mail Absentee Rejection Rate for Rejections NOT Associated with SB 1	2020 Total Mail Absentee Ballot Rejection Rate	Increase in Total Rejection Rate from 2020 to 2022, including Only Rejections in 2022 NOT Due to SB 1
	(1)	(2)	(3) = (2)/(1)	(4)	(5) = (3)/(4)
Dallas	20,914	347	0 0166	0 0061	2 7
Harris	64,625	558	0 0086	0 0031	2 8
Hidalgo	5,059	34	0 0067	0 0108	0 6
Totals	90,598	939	0 0104	0 0044	2 4
Source:	Mayer Table 1	Computed from Mayer Table 2	-	Mayer Table 1	-

Notes: 2022 Total votes cast was computed as the number of cumulative in-person votes reported on the Secretary of State Website at https://earlyvoting texas-election com/Elections/getElectionEVDates do at the close of business on election day in 2022, plus the number of mail absentee ballots returned reported in Column 1 of Table 1 from Mayer

14. Finally, Professor Mayer seems to give little thought to whether the mail ballot rejection rates are likely to remain similar to those observed in the November 2022 election. This is important because anytime a new rule is introduced, there is a learning curve for both election administrators and voters. This was evident in the fact that the mail-in ballot rejection rate for all of Texas fell from 12.4 percent to 2.7 percent from the 2022 primary election to the 2022 general election.[1] It was also explicitly acknowledged by the Brazos County Elections Administrator, who indicated that many of the mail-in ballot rejections for the November 2022 election she had seen at the time of the interview were from voters who had not yet learned, despite election administrators' best efforts, to follow the new rules.[2] As a result, it is possible, if not likely, that ballot rejection rates will decline going forward, compared to the November 2022 election.

[1] Rejections of Texans' mail ballots decline markedly from big surge in March primary (dallasnews.com)

[2] Of the voters who had voted in the primary, Ms. Hancock said "This is a new process for them. Those who voted in the primary and the runoff know what they need to do, as opposed to someone who is only voting in the November election." *See* https://www.texastribune.org/2022/10/20/voting-texas-ballot-rejections/.

C. Professor Mayer's Assertions that SB 1 Lacks Any Legitimate Justification

15. After (mis)interpreting the 2,949 ballot rejections among the 1,881,445 ballots cast and counted as evidence of disenfranchisement, Professor Mayer asserts that SB 1's mail absentee ballot rules are "pure deadweight", and that "[t]he new mail absentee ballot rules, moreover, do not contribute to election security or the integrity of the voting process".

16. The first problem with these assertions is that as demonstrated above, the evidence that Professor Mayer chose to interpret as evidence of disenfranchisement could just as easily be interpreted as evidence of a reduction in fraud. In this way, his rationale for asserting that SB 1 does not reduce fraud is based entirely on his own (mis)interpretation of rejected votes as unambiguous evidence of disenfranchisement, rather than fraud.

17. In addition, much if not all of Professor Mayer's argument seems to hinge on his mistaken belief that absence of evidence is evidence of absence. For example, in concluding that election fraud is rare, he cites both the number of prosecuted fraud cases in Texas, as well as a study by Auerbach and Pierson (2021) that used data on the number of election fraud cases prosecuted nationally. The trouble, of course, is that it is difficult to detect and measure election fraud in general, and the type of fraud that would be prevented by SB 1's mail-in voting rules in particular. Professor Mayer acknowledges this himself in his own study, where he states that "the prevalence of fraudulent voting, as with any illegal or largely private matter, is difficult to measure."[3] Given that reality, how could we have any confidence that mail-in voter fraud would be detected if it occurred?

18. While it is hard to know the answer to that question, we can assess how the incidence of other types of crime compares with measures of crime prosecution. It turns out, for example, that less than seven percent of property crimes are both reported and prosecuted or otherwise cleared by

[3] *See* Ahlquist, Mayer, and Jackman (2014), which can be accessed at https://www.liebertpub.com/doi/abs/10.1089/elj.2013.0231.

police.[4] In fact, even for the highest-priority criminal incidents—violent crime—only 21.4 percent are prosecuted. Yet it would be a mistake to pretend that those incidents that were not prosecuted never happened.

19. Importantly, property crimes are very likely to be detected by the victim, and violent crimes are almost certain to be detected by the victim. In contrast, mail-in voting fraud is unlikely to be detected by its victims or election personnel. This raises an important question: If prosecutions for property and violent crimes are that low, when the crime is almost certainly detected, when there is a victim who has an incentive to report the crime, and when there is often considerable evidence, wouldn't we expect the prosecution rate for mail-in voting fraud to be much, much lower? To be clear, I do not claim to know whether unreported mail-in voter fraud is common, or nonexistent. Rather, the prudent view is that it is difficult or even impossible for anyone to know with any degree of certainty.

20. Moreover, even if one believes there is zero election fraud, it is clear the American public strongly disagrees. For example, a 2016 survey by the Associated Press and the NORC Center for Public Affairs Research at the University of Chicago indicates that 35 percent of Americans say there is a "great deal" of election fraud in the United States, 39 percent say there is "some election fraud", and only 24 percent say there is "hardly any".[5] Similarly, a 2020 survey administered by California Institute of Technology found that 15 percent of registered voters believe that absentee ballot impersonation is "very common", and another 18 percent believe there is "occasional" absentee ballot impersonation.[6] It is hard to imagine that anyone, including Professor Mayer, would dispute that these perceptions are bad for democracy. As a result, one clear potential benefit of administering

---

[4] *See* https://www.pewresearch.org/fact-tank/2017/03/01/most-violent-and-property-crimes-in-the-u-s-go-unsolved/, which indicates that only 34.6 percent of property crimes are even reported to the police, and only 19.4 percent of those are cleared (0.346 * 0.194 = 0.067).

[5] *See* https://apnorc.org/projects/views-on-the-american-election-process-and-perceptions-of-voter-fraud/.

[6] *See* https://static1.squarespace.com/static/5ace8a6b45776eba2e40cbee/t/5fbbe565ae796c5cf419b024/1606149477949/Perception_of_Fraud_2020_Survey_Report.pdf.

safeguards such as those adopted by SB 1 is they may reduce these perceptions of widespread election fraud.

V. The Errors in Professor Mayer's Disparate Impact Analysis

21. The doctrine known as "disparate impact" pertains to insufficiently justified practices that, while facially neutral with respect to factors such as race or ethnicity, have the impact of harming some protected groups more than others. In his report, Professor Mayer asserts that SB 1's safeguards on absentee voting "disproportionately burden non-White voters". If true – and if one were to believe Professor Mayer's unfounded declarations that SB 1's safeguards provide zero benefit and are thus insufficiently justified – this would indeed be troubling.

22. The problem with Professor Mayer's analysis is threefold. First, Professor Mayer computed disparate impact incorrectly. Fixing this mistake in Harris County reveals that SB 1 had a disparate impact *against Whites,* relative to Hispanics, and had no disparate impact against Asians. Second, Professor Mayer fails to account for the fact that he does not observe actual race or ethnicity, for any Texan voters. Rather, he uses measures that represent educated guesses based on surname and residential neighborhood. The resulting classification error is significant. Thus, while Professor Mayer (and I) can estimate differences by predicted race, I demonstrate that this tells us little with certainty about differences by actual race. Third, Professor Mayer seems to ignore the issue of whether Texas voters consider alternative forms of voting as good substitutes for mail-in voting. This is important, since that issue is essential to assessing burden. I describe each of these three problems in turn.

A. Professor Mayer's incorrect method of computing disparate impact in Harris County and Dallas County

23. In assessing whether SB 1's safeguards on mail-in voting had a disparate impact on minority voters in Harris County, Professor Mayer examines only the voters in 2020 who chose to

vote via mail-in ballot. He then asserts that the results in Table 3 of his report, which show higher mail ballot rejection rates for voters predicted to be minorities, are evidence of disparate impact.

24. Professor Mayer's focus only on those voters who chose to vote absentee is incorrect. Table 3 provides an example that illustrates the problem. It shows voting behavior for two equal-sized groups: Group A, and Group B, after a hypothetical mail-in voting rule is adopted.[7] All 1,000 members of Group A chose to vote by mail. Ten percent, or 100, of those votes were subsequently rejected. By comparison, of the 1,000 members of Group B, only 2 voted by mail, 1 of whom was rejected.

Table 3: Illustration of How Computing Disparate Impact Incorrectly, as Professor Mayer Did, Can Lead to Incorrect Conclusions

Race	Total Votes Cast	Total Mail Ballots Cast	Mail Ballots Accepted	Mail Ballots Rejected Due to ID Rules	% Mail Ballots Rejected (Incorrect Method Used by Mayer)	Incorrect Odds Ratio for Computing Disparate Impact Used by Mayer	% Ballots Rejected (Correct Method of Measuring Disparate Impact)	Correct Odds Ratio for Computing Disparate Impact
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group A	1,000	1000	900	100	10.0%	-	10.0%	-
Group B	1,000	2	1	1	50.0%	5.0	0.1%	0.010

25. It is clear from this example that the hypothetical mail ballot identification rule had a disparate impact on Group A, relative to Group B. That is because while the rejection rate among absentee voters was higher in Group B than Group A (50 percent versus 10 percent), members of Group A voted absentee at much higher rates. The importance of considering the latter is obvious: while only 1 out of 1,000 members of Group B was unable to vote due to the hypothetical identification rules, 100 out of 1,000 members of Group A were unable to do so. Thus, the correct odds ratio of 0.01 is shown in Column (8), which is the proportion of all ballots rejected for Group

[7] Assume, for the sake of simplicity, that no mail-in ballots were rejected prior to the adoption of the hypothetical rule, and that all of the votes cast were legitimate.

B, divided by the same rate for Group A (both shown in Column (7)). This indicates that Group B's rejection rate is only one percent as large, or 99 percent lower, than Group A's rejection rate.

26. Yet under Professor Mayer's flawed approach used in Tables 3 through 6 of his report, he would mistakenly conclude that there was disparate impact against Group B (only one member of which was prevented from voting), relative to Group A (which had 100 members prevented from voting). This is because he considers only the members of Groups A and B who voted absentee, and ignored the prevalence of voting method across groups.

27. Notably, the importance of considering the prevalence of voting method by group in assessing disparate impact has been acknowledged by at least two of the plaintiff's experts, including Professor Mayer himself. For example, on page 11 of Professor Mayer's March 6 report, he states, with respect to drive-thru voting, that "voters of color in Harris County disproportionately relied on drive-thru voting during the 2020 general election, and that eliminating the method of voting imposes a disproportionate burden on such voters." Similarly, another expert for the plaintiff, Professor Eric McDaniel, stated in line 266 of his February 28, 2022 report that "Blacks are more likely to vote absentee than their White or Hispanic counterparts, meaning that the new regulations will increase the difficulty of them doing so in the future." While both of these statements are likely problematic for other reasons—for example, I demonstrated in my response that Professor McDaniel ignored data for years that showed the opposite pattern—both were correct in asserting the importance of the prevalence of voting method across groups in assessing disparate impact.

28. Table 4 shows that correcting this error implies that there is no longer any evidence of disparate impact against voters predicted to be Hispanic, or predicted to be Asian, in Harris County. Column (1) shows the total number of votes cast, per the data provided to me by Professor Mayer.[8]

[8] I was not able to determine whether these included the mail-in ballots or not, since Professor Mayer did not provide me with those data. However, in practice it matters little either way for the purpose of this table, since the number of mail-in ballots is small relative to total votes cast.

Column (2) shows the number of mail-in votes rejected, as reported by Professor Mayer, and Column (3) reports the fraction of total votes rejected for reasons associated with SB 1. As shown there, only 0.2 percent of ballots cast by predicted-White voters were rejected, compared to 0.4, 0.16, and 0.2 percent of ballots cast by predicted-Black, predicted-Hispanic, and predicted-Asian voters in Harris County.

29. The correct odds ratios, or the rejection rate for predicted-minority groups divided by that for predicted-Whites, are shown in Column (4). The incorrect odds ratios reported by Professor Mayer are reported in Column (5). It shows that when computed properly, there is evidence of disparate impact *against predicted-Whites* relative to predicted-Hispanics, and no evidence at all of disparate impact against predicted-Asians. In contrast, the evidence of disparate impact against predicted Blacks is somewhat larger than Professor Mayer reported (2.09 versus 1.82), though in the next section I discuss the significant problems associated with predicting race in general, and in particular for Black Americans.

Table 4: Computing Correct Odds Ratios for Computing Disparate Impact (Harris County 2022 General)

	Total Votes Cast	Mail-In Ballots Rejected due to SB 1, per Mayer	Proportion Total Votes Rejected for SB 1 Reasons	Correct Odds Ratios (Relative to White) for Computing Disparate Impact	Incorrect Odds Ratios (Relative to White) for Computing Disparate Impact, Reported by Mayer
	(1)	(2)	(3)	(4)	(5)
Predicted White	626,680	1,251	0.00200	-	-
Predicted Black	187,416	782	0.00417	2.09	1.82
Predicted Hispanic	222,556	357	0.00160	0.80	1.86
Predicted Asian	61,989	127	0.00205	1.03	1.71
Predicted Other	4,256	18	0.00423	2.12	2.13
Total	1,102,897	2,535	0.00230	-	-

Notes: Column (1) counts all individuals indicated to have voted per the data Professor Mayer provided to me Column (2) is taken directly from Table 3 of Mayer, while Column (5) is taken from Table 4 of Mayer The difference in rejection likelihood is not different for Predicted-Asian versus Predicted-White; in all other cases the differences are statistically significant

30. Unfortunately, I am unable to report corrected disparate impact estimates for Dallas County. This is because doing so requires individual-level data, with predicted race or ethnicity, for all voters in Dallas County. In contrast, the data Professor Mayer shared with me include only the names of absentee voters in Dallas County.

31. However, the results of the illustration shown in Table 3 above, and the results of the correct analysis for Harris County shown in Table 4 above, make it clear that we learn little about likely disparate impact from the incorrect analyses shown in Professor Mayer's report. In particular, without knowing the rate at which predicted-Black and predicted-Hispanic voters in Dallas County voted absentee, one is unable to assess whether SB 1's rules regarding identification may have had a disparate impact on either group, relative to predicted-Whites.

B. The Impact of the Large Classification Error When Using Predicted Race or Ethnicity, Rather than Actual Race or Ethnicity

32. It is easy to get the mistaken impression from Professor Mayer's report that he observes voting outcomes by race and ethnicity. He does not. Rather, he is relying on a measure of race or ethnicity that comes solely from a voter's surname, combined with the Census characteristics of the Census Block Group (i.e., neighborhood) of the voter's residence.

33. The problem, of course, is that this method of inferring race or ethnicity is far from perfect. There are two types of classification errors. The first, called Type I error, or "false positive rate", is when the algorithm predicts that an individual belongs to an ethnic group when that is not true in reality. For example, the published paper by the researchers who developed this algorithm note that 4.28 percent of individuals predicted to be Black, are not actually Black.[9] An even bigger

[9] *See* Table 5 in "Addressing Census Data Problems in Race Imputation via Fully Bayesian Improved Surname Geocoding and Name Supplements" by Kosuke Imai, Santiago Olivella, and Evan T. R. Rosenman, which was published in Science Advances on December 9 of 2022. The paper is accessible at https://www.science.org/doi/10.1126/sciadv.adc9824.

problem is Type II error, or "false negatives". The published estimate implies that of those individuals predicted to *not* be Black, 17.77 percent are actually Black.

34. How do classification errors of this magnitude impact our ability to estimate disparities across actual race? The typical way of addressing this type of issue is to perform what social scientists refer to as a bounding exercise, where we compute the range of possibilities that could arise under different assumptions about the classification error. If classification errors are small and matter little, then both bounds are similar, which gives us confidence that the answer is unaffected by the assumptions one makes about that error. On the other hand, if the range between the bounds is wide—as turns out to be the case for this setting—it implies we know little about the correct answer to the question.

B.1 Harris County

35. Table 5 shows the results from a bounding analysis for Harris County. I focus only on the rejection rate for predicted-Black voters in Harris County, since that was the one group for which the correct analysis in Table 4 suggested there may be evidence of disparate impact. Panel A indicates that there were 187,416 voters in Harris County in 2022 who were predicted to be Black. Of those, 782 had mail-in ballots rejected as a result of the identification safeguards associated with SB 1. As noted above, however, our best estimate is that 4.28 percent of those voters were not actually Black. This suggests that of the 187,416 predicted-Black voters, only 179,395 were actually Black.

36. Of those, how many had their absentee ballots rejected due to SB 1? The answer is that there is no way for me, or Professor Mayer, to know. The true answer is that perhaps all 782 rejections, and perhaps none. Columns (4) and (5) show the lower and upper bound for rejection rates, which are 0.0000 and 0.0044. Put differently, while Professor Mayer knows the ballot rejection rate for voters predicted to be Black, he does not know the ballot rejection rate for the subset of those voters who are actually Black. Rather, the only thing we know for sure is that the rate is between 0

and 0.0044, and the odds ratio relative to predicted-White voters is between 0 (Column (6)) and 2.18 (Column (7)).

37. Panel (B) of Table 5 shows that the problem is even worse with respect to the other type of classification error. In particular, of those voters predicted to NOT be Black—which in the case of Harris County is 915,481 voters—the researchers who constructed the algorithm believe 17.77 percent are actually Black. Given 915,481 voters in Harris County were predicted to not be Black, this means that the algorithm misclassifies 162,681 Black voters in Harris County as some race or ethnicity other than Black.

38. Put differently, the authors of the algorithm expect that there are 342,076 Black voters in Harris County. Yet the algorithm only classifies 187,416 as Black, and several thousand of those aren't actually Black.

Table 5: Impact of Errors in Predicting Race on the Range of Possible Rejection Rates of Black Voters in Harris County

	Total Votes	# Rejected Mail-In Ballots per SB 1	Rejection Rate			Odds Ratio: Actual Black/ Predicted White	
			Actual	Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Voters Predicted to be Black							
Predicted Black, 4.28 percent of whom are not actually Black	187,416	782	0.0042	-	-	-	-
Actual Black voters who were predicted to be Black	179,395	Unknown; between 0 and 782	Unknown	0.0000	0.0044	0.00	2.18
Panel B: Voters Predicted Not Black							
Predicted Not Black, 17.77 percent of whom are actually Black	915,481	1,753	0.0019	-	-	-	-
Voters predicted as not Black, but who are actually Black	162,681	Unknown; between 0 and 1,753	Unknown	0.0000	0.0108	0.00	5.39
Panel C: Actual Black Voters							
Actual Black Voters	342,076	Unknown; between 0 and 2,535	Unknown	0.0000	0.0074	0.00	3.71

Notes: Per the study by Imai, Olivella, and Rosenman (2022) published in *Science Advances*, 4.28 percent of voters predicted to be Black are actually Black, while 17.8 percent of voters predicted not to be Black are actually Black. These error rates are used in Column (1) to estimate the number of actual Black voters. Odds ratios in Columns (6) and (7) are computed by dividing the rejection rate in Columns (4) and (5) by the rejection rates for voters predicted to be White, which was 0.002.

39. It should be obvious that the voting behavior of the 162,681 expected-Black voters who are misclassified as non-Black will have a major impact on the actual ballot rejection rate of Black voters. How wrong can the estimates for predicted-Black voters be? It depends entirely on how many of those misclassified Black voters had their ballots rejected. The only thing anyone can know for sure is that the true number is somewhere between 0 (if all of the 1,753 rejections were for non-Black voters) and all of them (i.e., 1,753).

40. Panel C sums up the net impact of both of these types of errors. Even though the algorithm predicted that only 187,416 voters were Black, based on the published error rates there were actually an estimated 342,076 actual Black voters. Those Black voters were associated with somewhere between 0 and 2,535 rejected mail-in ballots. That means that rejection rate could be zero, which is obviously less than that of predicted-Whites, or 0.0074, which is 3.71 times the rate for predicted-Whites. There is no way to know, without imposing impossible-to-justify assumptions, where in that range the true disparity lies.

B.2 Dallas County

41. Professor Mayer also concludes that SB 1 generated a disparate impact against predicted-Black and predicted-Hispanic voters in Dallas County. As noted above, and as with Harris County, he computed disparate impact incorrectly. In particular, he ignored the possibility that different groups can vote absentee at different rates, even though that is a major factor in assessing whether a group is disproportionately impacted by additional voting rules.

42. In addition, as in his analysis of Harris County, Professor Mayer only provides evidence regarding racial disparities between predicted-Black or predicted-Hispanic voters, compared to predicted-White voters. Again, this begs the question: what do these estimates tell us about the differences between voters whose actual race or ethnicity is different?

43. As in Harris County, the answer is "very little". Tables 6 and 7 show the same type of

bounding analysis for Dallas County. The difference between these tables, and Table 5 for Harris County, is that in Dallas County I am unable to compute disparate impact correctly. This is because I do not have voter-level data, including residential address, on every voter who cast a ballot in the 2022 election in Dallas County. As a result, I instead perform a bounding exercise for disparate impact estimates computed in the same (incorrect) way as Professor Mayer.

44. Results in Tables 6 and 7 show that as in Harris County, once we take into account the known error rates in classifying race and ethnicity, we know little about the relative rejection rates of actual White mail-in voters compared to those who are actually Black, or Hispanic. Again, the intuition is straightforward. Column (2) of Panel A of Table 6 shows that Professor Mayer predicts that 4,230 mail-in voters in Dallas County were Black. The biggest problem is that there were actually an estimated 7,014 actual Black voters who cast mail-in ballots. Nearly 3,000 Black voters were improperly predicted to be White. What hope do we have of computing the correct ballot rejection rate for Black mail-in voters when we are misclassifying nearly half of them?

45. Similarly, for Hispanics, Column (2) of Panel A of Table 7 shows that Professor Mayer predicts there were 1,550 ballots cast by Hispanic voters. Again, the biggest problem is that there were another 2,277 actual Hispanic voters who were misclassified as being non-Hispanic. Put differently, in computing the ballot rejection rate of predicted-Hispanics, Professor Mayer is ignoring the rejection rate of more than half of Hispanics who cast mail-in ballots. Any reasonable observer ought to question the reliability of this method as a way of computing disparate impact across racial or ethnic groups.

46. Column (7) of Tables 6 and 7 show that once we take into account the significant classification error, it is possible that the ballot rejection rates of actual Black and Hispanic voters are much lower—even zero—compared to the ballot rejection rate of actual White voters. Put differently,

we learn very little about racial disparities across actual race or ethnicity by comparing across individuals who are predicted to be of different race or ethnicity.

Table 6: Impact of Errors in Predicting Race on the Range of Possible Rejection Rates of Black Voters in Dallas County

	Total Votes	# Mail-In Ballots	# Rejected Mail-In Ballots per SB 1	Rejection Rate among Mail-in Ballots			Odds Ratio: Actual Black/ Predicted White	
				Actual	Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Voters Predicted to be Black								
Predicted Black, 4 28 percent of whom are not actually Black	Unavailable	4,230	101	0 0239	-	-	-	-
Actual Black voters who were predicted to be Black	Unavailable	4,049	Unknown; between 0 and 101	Unknown	0 0000	0 0249	0 00	1 95
Panel B: Voters Predicted Not Black								
Predicted Not Black, 17 77 percent of whom are actually Black	Unavailable	16,684	219	0 0131	-	-	-	-
Voters predicted as not Black, but who are actually Black	Unavailable	2,965	Unknown; between 0 and 219	Unknown	0 0000	0 0739	0 00	5 78
Panel C: Actual Black Voters								
Actual Black Voters	Unavailable	7,014	Unknown; between 0 and 320	Unknown	0 0000	0 0456	0 00	3 57

Notes: Per the study by Imai, Olivella, and Rosenman (2022) published in *Science Advances*, 4 28 percent of voters predicted to be Black are actually Black, while 17 8 percent of voters predicted not to be Black are actually Black These error rates are used in Column (2) to estimate the number of actual Black voters Odds ratios in Columns (7) and 8) are computed by dividing the rejection rate in Columns (5) and (6) by the rejection rates for voters predicted to be White, which was 0 01278

Table 7: Impact of Errors in Predicting Race on the Range of Possible Rejection Rates of Hispanic Voters in Dallas County

	Total Votes	# Mail-In Ballots	# Rejected Mail-In Ballots per SB 1	Rejection Rate among Mail-in Ballots			Odds Ratio: Actual Hispanic/ Predicted White	
				Actual	Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Voters Predicted to be Hispanic								
Predicted Hispanic, 2 11 percent of whom are not actually Hispanic	Unavailable	1,550	27	0 0174	-	-	-	-
Actual Hispanic voters who were predicted to be Hispanic	Unavailable	1,517	Unknown; between 0 and 27	Unknown	0 0000	0 0178	0 00	1 39
Panel B: Voters Predicted Not Hispanic								
Predicted Not Hispanic, 11 76 percent of whom are actually Hispanic	Unavailable	19,364	293	0 0151	-	-	-	-
Voters predicted as not Hispanic, but who are actually Hispanic	Unavailable	2,277	Unknown; between 0 and 293	Unknown	0 0000	0 1287	0 00	10 07
Panel C: Actual Hispanic Voters								
Actual Hispanic Voters	Unavailable	3,795	Unknown; between 0 and 320	Unknown	0 0000	0 0843	0 00	6 60

Notes: Per the study by Imai, Olivella, and Rosenman (2022) published in *Science Advances*, 2 11 percent of voters predicted to be Hispanic are actually not Hispanic, while 11 76 percent of voters predicted not to be Hispanic are actually Hispanic These error rates are used in Column (1) to estimate the number of actual Hispanic voters Odds ratios in Columns (7) and (8) are computed by dividing the rejection rate in Columns (5) and (6) by the rejection rates for voters predicted to be White, which was 0 01278

B.3 Additional considerations casting doubt on the reliability of using educated guesses about race and ethnicity to assess disparate impact

47. As though the results in Tables 5 – 7 are not bad enough news for the reliability of this type of analysis, the reality is even worse. Tables 5 – 7 only consider the error in computing the ballot rejection rate of Black or Hispanic voters. Yet disparities are constructed by comparing that rate for (actual) Blacks to the rate for (actual) Whites. Unsurprisingly, there is also considerable error in predicting whether a voter is White. Additionally adjusting for this would further increase the likelihood that comparisons across predicted-race are uninformative of reality.

48. In addition, the algorithm was constructed using data from other states, and did not include Texas. To the extent that Texas is different from those states, we would expect the classification error rates to be even higher than what was documented by Imai, Olivella, and Rosenman (2022).[10]

49. In fact, there is evidence that out-of-sample error rates are much higher than those reported by Imai, Olivella, and Rosenman (2022), which I used in Tables 5 - 7. For example, the American Financial Services Association commissioned a report by Charles River Associates in November of 2014.[11] As part of that report, the authors assessed the classification error rate using the BISG method also used by Professor Mayer. They did this by comparing predicted race to actual self-reported race, and therefore test the reliability of the BISG method used by Dr. Loren Collingwood, and provided to Professor Mayer.

50. The results are alarming. Table 8 of that Charles River Associates report shows that when using a threshold of 50 percent (e.g., an individual is classified as Black if the probability of Black

[10] It is also unclear to me whether Professor Mayer uses predictions based on the rBISG method described in the Imai, Olivella, and Rosenman (2022) study, or the less accurate BISG method. I assume they are using the former, but if they are using the latter, my analysis understates the impact of classification error.

[11] The report is entitled "Fair Lending: Implications for the Indirect Auto Finance Market". It can be accessed at https://www.crai.com/insights-events/publications/fair-lending-implications-indirect-auto-finance-market/.

is greater than 50 percent), 43.6 percent of those classified as Black are not actually Black. They also show that 51.8 percent of actual Black individuals are incorrectly classified as being not Black.

51. The results of that study also demonstrate that while it is possible to use alternative rules to reduce one type of classification error, it comes at the cost of increasing another.[12] For example, Table 8 of the same report shows that if one instead uses a threshold of 80 percent, it reduces the number of non-Black individuals mistakenly predicted to be Black from 43.6 percent to 22.4 percent. However, the same change increases the fraction of Black individuals who are mistakenly classified as non-Black from 51.8 percent to 75.8 percent.

52. In short, the problem is that surname and residential neighborhood simply aren't very good at predicting actual self-reported race or ethnicity. As a result, it is hard to infer anything about disparities across actual race or ethnicity using that approach.

C. Professor Mayer's analyses of disparate impact ignore the fact that Texan voters are willing and able to substitute from mail-in voting to in-person voting

53. Leaving aside the issue of whether the identification safeguards imposed by SB 1 are justified, an important factor in assessing whether voters are burdened by SB 1 depends on the extent to which they are willing and able to vote using another method. For example, if voters viewed in-person voting as a perfect substitute for mail-in voting, then even the strictest regulation—such as the elimination of all mail-in voting—would not have adverse effects on voters. On the other hand, if voters in Texas view voting in person as a poor substitute for voting absentee, some may decide not to vote at all in response to the mail-in ballot requirements of SB1.[13]

[12] The other alternative is to exclude individuals for whom there is an especially low degree of certainty about their actual race, though this is also problematic for obvious reasons.

[13] This is the same issue that economists face in assessing tax incidence, which involves identifying which party in the market bears the burden of a tax. While one might be inclined to believe that individuals who consume the good being taxed will bear the burden, this need not be the case. In particular, if those consumers have a perfect substitute available to them, they will simply shift consumption and bear none of the tax burden.

54. Despite the fact that substitutability is central to establishing burden, Professor Mayer never discusses the issue in his report. The only reference to voters substituting or not substituting across methods is in a single footnote, which mentions less than 500 voters. Yet this is a critical issue for assessing whether SB 1 generated a burden on voters, or a disparate burden on minority voters.

55. The best way to assess substitutability is to use an approach that mirrors the following thought experiment: If we were to make absentee voting easy for a random set of voters, but more difficult for otherwise similar voters, would they vote at similar rates? Put differently, would voters substitute toward voting in person on a one-to-one basis, or would some voters be so burdened that they no longer participate?

56. Coincidentally, the best study on this topic, which was published in *Science Advances* in 2021, mirrors this thought experiment using data from Texas (and Indiana). Intuitively, the authors compare those who just turned 65 prior to the election and could easily vote absentee to those who turned 65 just after the election, and could not. Notably, this is a much more extreme hurdle to mail-in voting than any caused by SB 1. Yet while eligible voters did shift toward absentee voting, this increased turnout was offset in a one-to-one fashion by a shift away from in-person voting. In short, the ability to vote absentee only impacts *how* Texans (and Hoosiers) vote, not *whether* they vote. Put differently, the results indicate even a version of SB1 that made absentee voting nearly impossible for everyone would not impose a sufficiently large burden as to prevent Texans from voting.

57. The basic result of the study is shown in Figure 1 which replicates Figure 2 from the original paper. It shows that while there were clear jumps in the likelihood of voting by mail at age 65 in the 2012, 2016, and 2020 elections, there was no such jump in overall voter turnout. In other words, voters seemed to substitute from mail-in voting to in-person voting, on a one-to-one basis, when they were not allowed to vote by mail (i.e., when they were not yet 65 years old).

58. Notably, the change in the ability to vote absentee studied in that paper—where very few 64 year-olds are eligible to vote by mail—is much more extreme than the changes imposed by SB1. As a result, one must ask: If even an extreme change in the ability to vote by mail does not burden Texas voters so much that they do not vote, then why would we expect the much smaller changes imposed by SB1 to impose a burden?

59. It is not clear from Professor Mayer's report whether or not the mail-in ballot rejections he considers are final rejections in which the individual did not subsequently vote using any method. In particular, it is unclear whether the 2,949 voters Professor Mayer identifies as having had their ballot rejected were able to vote successfully by either curing their ballot, or voting using another method.

60. However, what is clear is that across all of Texas in the 2022 election, there were only 6,355 mail-in ballots, out of 8.1 million votes cast, that were rejected, and where the voter did not subsequently vote successfully by mail or another method. This evidence comes from the February 3, 2023 report of another expert for the plaintiff, Professor Eitan Hersh, and is documented in my response to that report. Importantly, there is no evidence in Professor Hersh's report or in the underlying data to suggest that these votes were legitimate rather than illegitimate votes. Similarly, there is no evidence to suggest voter errors were not responsible for some or all of the rejections, which may well become less common as voters become more familiar with the new rules. Professor Hersh's finding is consistent with the evidence shown in Figure 1 from the *Science Advances* study, which shows that even severely restricting mail-in voting only changes *how* Texans vote, not *whether* they vote.

Figure 1: A replication of Figure 2 from Yoder et al. (2021)

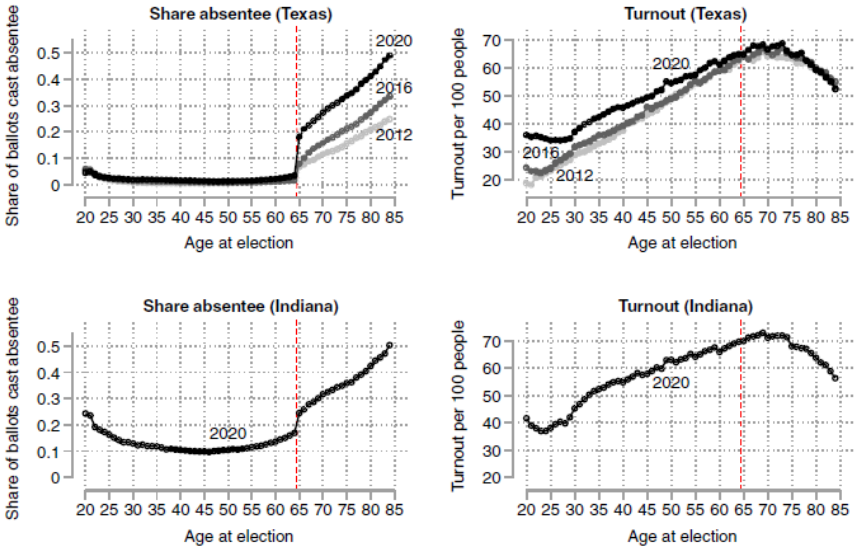


Fig. 2. Absentee voting and turnout across age and elections in Texas and Indiana. In Texas and Indiana, only voters aged 65 or older can vote absentee without providing an excuse. This creates a large and discontinuous increase in voting absentee for 65-year-olds, which grew markedly in 2020 during the pandemic. Yet, turnout does not increase discontinuously between age 64 and 65, implying that the discontinuous increase in absentee voting is offset by a reduction in other modes.

Yoder *et al.*, *Sci. Adv.* **7**, eabk1755 (2021) 22 December 2021 **3 of 8**

VI. Professor Mayer's incorrect assertion that the elimination of drive-thru voting had a disproportionate effect on non-White voters

61. Professor Mayer asserts that "the elimination of drive-thru voting had a disproportionate effect on non-White voters who cast early drive-thru votes in the 2020 general election." In this section, I demonstrate that Professor Mayer's assertion is incorrect.

62. I do so in three ways. First, I show that there is no evidence of an adverse effect of SB 1 on the subsequent voting of drive-thru voters, either in the aggregate or for any predicted-race or ethnic group. Second, I show that implementing the same statistical test of "disparate impact" that Professor Mayer used yields nonsensical results. In particular, I show that if one uses that test on non-drive-thru early voters from Calhoun County in 2020, one would conclude that SB 1 had a disparate impact on them as well. Yet those voters' voting methods were clearly unaffected by SB 1. Finally, Professor Mayer's use of predicted measures of race and ethnicity, as in his analysis of Harris County

and Dallas County, makes it difficult to infer much of anything regarding differences across actual race or ethnicity.

A. Disparate impact implies there is an adverse effect on voters, and there is no evidence that drive-thru voters were adversely impacted either in the aggregate, or for any predicted-race or ethnic group

63. As background, it is helpful to remember the meaning of "disparate" or "disproportionate" impact, as it applies to the setting of drive-thru voting. In particular, the concern is that a facially neutral policy has a larger adverse impact on some protected groups than others.

64. What would it mean for the elimination of drive-thru voting to have an adverse effect on voters? If, for example, drive-thru voters were unable to vote using an alternative method, we would expect to see these voters vote at much lower rates than their counterparts who also voted early during the 2020 general election.

65. The problem is that there is zero evidence that the drive-thru voters in Calhoun County voted at lower rates than their non-drive-thru voting counterparts. Results are shown in Figure 2. It shows that in the 2022 primary election and the 2022 general election, drive-thru voters from 2020 voted at *higher* rates than their counterparts who voted early and in-person during the 2020 election. Put differently, there is no evidence that the elimination of drive-thru voting had any adverse effect at all on those who used drive-thru voting in 2020.

66. Table 8 shows the full set of results. Column (1) shows results from data provided by Professor Mayer for the voting rates of individuals from Calhoun County who voted in 2020 using the drive-thru option.[14] In Column (2), I replicate these findings using data I downloaded from the Texas Secretary of State website. Results are not identical, but are very similar. For example, Professor Mayer has a sample of 2,110 voters, 42.4 percent of whom voted in the 2022 primary election, and

[14] The voting rate in the 2022 election, as recorded in the data provided to me by Professor Mayer, does not match the voting numbers reported in Table 8 of Professor Mayer's report. I believe that the latter are incorrect, and therefore in Column (1) of Tables 8 and 9 I use the numbers from the data provided to me by Professor Mayer.

72.6 percent of whom voted in the 2022 general election. By comparison, I have a sample of 2,173 drive-thru voters from 2020, 42.0 percent of whom voted in the 2022 primary, and 72.2 percent of whom voted in the 2022 general election.[15]

Table 8: Fraction of 2020 General Election Drive-Thru and Other Early Voters in Calhoun County who Voted in 2022 Elections

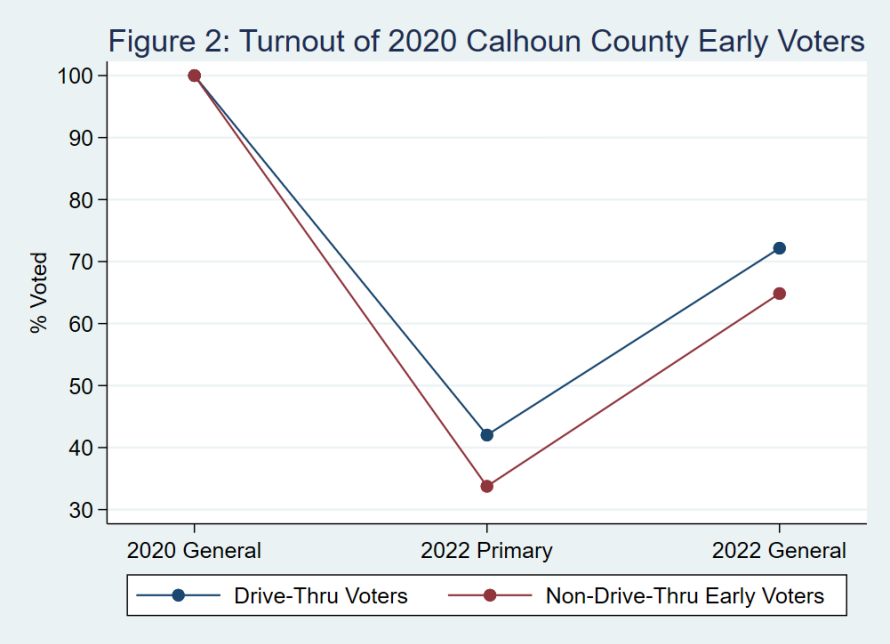
	2020 Drive-Thru Voters (Mayer's data)	2020 Drive-Thru Voters	2020 Early In-Person Voters, excluding drive-thru	2020 Early In-Person + Mail-In Voters, excluding drive-thru	Difference between Drive-Thru and Other Early In-Person Voters	Difference between Drive-Thru and Early In-Person + Mail-In Voters
	(1)	(2)	(3)	(4)	(5) = (2) - (3)	(6) = (2) - (4)
2020 General Election	1	1	1	1	0	0
2022 Primary Election	0.424	0.420	0.338	0.325	0.082***	0.095***
2022 General Election	0.726	0.722	0.648	0.629	0.073***	0.093***
# 2020 Voters	2,110	2,173	2,702	3,414	4,875	5,587
Sample	Prof. Mayer	TX SoS	TX SoS	TX SoS	TX SoS	TX SoS

Notes: Column (1) reports figures based on the data provided by Professor Mayer. The remaining columns report results based on data downloaded from the Texas Secretary of State website. *** denotes that the difference is statistically significant at the one percent level.

67. Columns (1) and (2) make clear that not all of those individuals who voted early, via drive-thru, in 2020 subsequently voted in the 2022 elections. This is likely in part because 2020 was a presidential election year, while 2022 was a midterm. In addition, turnout in 2020 was idiosyncratically high even by the standards of presidential election years.

[15] I was not able to assess the reasons for the differences. One potential explanation is that I believe that Professor Mayer obtained data directly from Calhoun County, whereas I used data from the Secretary of State website. Another difference is that because the Secretary of State records available online do not have residential address, I predicted race using only surname, along with the 2020 Census. Regardless, as can be seen in the first two columns of Table 8 and Table 9, results are similar across both data sets. The only difference is in Table 9 for predicted-Black voters, of whom there are less than 30, which means results are sensitive to the inclusion or exclusion of a handful of voters. Finally, if I were provided the full data directly from Calhoun County, as Professor Mayer was provided, I would be happy to replicate this analysis using those data.

68. The important question is whether fewer of these drive-thru voters turned out to vote in 2022 compared to others in Calhoun County who also voted early in the 2020 election. Column (3) shows results for those who voted early, in-person (excluding drive-thru voting), and column (4) shows results for those early voters plus those who voted by mail. In both cases, drive-thru voters from 2020 turned out at *higher* rates in 2022 than other early voters from Calhoun County. In short, it turns out that drive-thru voters demonstrated that they are more willing and able than other early voters from 2020 to vote in subsequent elections, even given the removal of the drive-thru voting option. This implies there is no evidence that the elimination had an adverse effect on voting.



69. Table 9 shows that the same is true for voters predicted to be in the various racial and ethnic groups.[16] In particular, Columns (5) and (6) indicate that predicted-White, predicted non-White, and predicted-Hispanic drive-thru voters were each more likely than their non-drive-thru early-voting

[16] The wru package in the statistical software R was used to predict probabilities of each racial or ethnic group using (only) surname, based on the 2020 Census. Using the code provided by Professor Mayer, I defined a voter as a certain predicted-race or ethnicity if the probability of being in that category was greater than 50 percent.

counterparts to vote in the 2022 primary and general elections. Again, this suggests that there is no evidence of adverse impact on any predicted-race or predicted-ethnic category.

Table 9: Fraction of 2020 General Election Drive-Thru and Other Early Voters in Calhoun County who Voted in 2022 Primary and General Elections

Race	2020 Drive-Thru Voters (Mayer's data)	2020 Drive-Thru Voters	2020 Early In-Person Voters, excluding drive-thru	2020 Early In-Person + Mail-In Voters, excluding drive-thru	Difference between Drive-Thru and Other Early In-Person Voters	Difference between Drive-Thru and Early In-Person + Mail-In Voters
	(1)	(2)	(3)	(4)	(5) = (2) - (3)	(6) = (2) - (4)
2020 General Election	1	1	1	1	0	0
2022 Primary Election - All Voters	0.424	0.420	0.338	0.325	0.082***	0.095***
Predicted White	0.493	0.482	0.398	0.375	0.084***	0.107***
Predicted Non-White	0.286	0.282	0.193	0.209	0.089***	0.073***
Predicted Black	0.273	0.407	0.385	0.371	0.023	0.036
Predicted Hispanic	0.310	0.298	0.188	0.211	0.110***	0.086***
Predicted Asian	0.129	0.134	0.174	0.144	-0.040	-0.010
2022 General Election - All	0.726	0.722	0.648	0.629	0.073***	0.093***
Predicted White	0.777	0.777	0.725	0.689	0.052***	0.088***
Predicted Non-White	0.625	0.597	0.471	0.490	0.126***	0.106***
Predicted Black	0.593	0.778	0.731	0.714	0.047	0.063
Predicted Hispanic	0.659	0.623	0.479	0.501	0.144***	0.123***
Predicted Asian	0.385	0.354	0.337	0.342	0.017	0.011
# 2020 Voters w/ predicted race	2,110	1,981	2,466	3,120	-	-
Sample	Prof. Mayer	TX SoS	TX SoS	TX SoS	TX SoS	TX SoS

Notes: Column (1) reports figures based on the data provided by Professor Mayer The remaining columns report results based on data downloaded from the Texas Secretary of State website *** denotes that the difference is statistically significant at the one percent level The analysis excludes individuals predicted to be of "other" race or ethnicity, since only such drive-thru voter in my data

B. Professor Mayer's flawed statistical test of disparate impact also mistakenly concludes there was disparate impact on Calhoun County 2020 early non-drive-thru voting minorities, whose method of voting was unaffected by SB 1

70. In addition, the problem with Professor Mayer's analysis can be demonstrated in another way. Professor Mayer relies on what is called a Chi-Square statistical test of whether the participation of minority drive-thru voters in the 2022 elections falls by more than for predicted-White

drive-thru voters. He concludes, on the basis of this test, that the elimination of drive-thru voting caused a disproportionate decline in the voter participation of predicted-minority voters.

71. Let's assume, for a moment, that this test is in fact a scientifically valid test of whether SB 1's elimination of drive-thru voting caused the larger decline in participation among predicted-minorities. If that were true, then if we apply the same test to a sample of 2020 Calhoun County voters who did not even participate in drive-thru voting, the test should give us a different answer. Formally speaking, we should not be able to reject the null hypothesis.

72. The problem is that when I perform the exact same statistical test as Professor Mayer, except on a sample of voters whose 2020 method of voting was completely unaffected by SB 1, I get the same result as Professor Mayer reported. In both cases, the test rejects the null hypothesis.[17] Yet it would be wrong to conclude that the elimination of drive-thru voting had a disparate impact, or even any impact at all, on 2020 voters who did not even use drive-thru voting. As a result, it is clear that the test Professor Mayer employed is an unreliable test. It is clearly poorly suited for assessing whether SB 1 caused a disparate impact on minorities.

C. Classification error rates in predicting race and ethnicity make it difficult to infer any differences between voters of actual different races or ethnicities

73. As noted in my discussion of Professor Mayer's disparate impact analyses of Harris County and Dallas County, Professor Mayer used educated guesses about the race and ethnicity of voters. Unfortunately, the published, known error rates associated with classifying race and ethnicity in this way imply that no small step of faith is required to believe that differences in predicted-race correspond to differences in actual race.

[17] For 2022 primary voting, the chi-squared statistic is 56.07, and the associated p-value is less than 0.001. For 2022 general election voting, the chi-squared statistic is 39.4, with a p-value of less than 0.001. This level of significance is qualitatively similar, and even somewhat more statistically significant, than that reported by Professor Mayer for 2020 drive-thru voters.

74. While I will not go through the same type of exercise I performed in Tables 6 and 7 for Harris County and Dallas County, a simple example illustrates the problems associated with predicting ethnicity in this way. For example, Table 8 of Professor Mayer's report indicates there were 588 drive-thru voters in the 2020 election predicted to be Hispanic. By comparison, there were 1,522 voters predicted to not be Hispanic. However, Table 8 of Imai, Olivella, and Rosenman (2022) indicates the algorithm has an 11.76 percent false negative rate for Hispanics. That suggests that of the 1,522 voters predicted to be non-Hispanic, 179 of them actually identify as Hispanic.

75. How would the inclusion of those individuals in the correct group (i.e., Hispanics) impact the analysis? It is impossible to know. That is because we do not know who they are, and therefore we do not know how they voted in the 2022 primary and general elections. This number of Hispanics could impact the calculations in a meaningful way, given only 182 predicted-Hispanics voted in the 2022 primary, and only 267 predicted-Hispanics voted in the 2022 general election. If all 179 of these mis-classified Hispanics in fact voted in the subsequent elections, it would mean that Professor Mayer significantly understates voting rates by Hispanics, and overstates the voting rates of the other groups – namely, Whites.

76. As a result, if Professor Mayer or anyone else wishes to speak to racial or ethnic disparities, they either need a much more accurate prediction system, or data on actual race and ethnicity.

VII. Conclusion

77. Professor Mayer would have you believe that SB 1 provides zero benefit to election security. He provides no evidence of this, except for citing the number of prosecuted cases of election fraud. Yet his own academic writings acknowledge the difficulty of detecting election fraud. Moreover, Professor Mayer ignores the fact that prosecutions massively undercount other types of

crime that have clear victims, and are more likely to be detected, reported, and solved. Finally, Professor Mayer ignores the potential benefit of adopting safeguards such as SB 1 in addressing the indisputable, widespread belief by many Americans, whether correct or not, that election fraud is a problem.

78. Professor Mayer would also have you believe that out of the 1,991,445 voters who cast ballots in Dallas, Harris, and Hidalgo counties in the 2022 general election, 2,949 voters were disenfranchised because their mail-in votes were rejected. Yet there is nothing in Professor Mayer's report, or in the underlying data, to suggest that these were legitimate votes. Put simply, Professor Mayer chooses to (mis)interpret any rejection of mail ballots as evidence of disenfranchisement, even though the same evidence is equally consistent with a reduction in illegitimate votes counted.

79. Professor Mayer would also have you believe that the identification safeguards adopted by SB 1 caused an increase in mail-in ballot rejections, relative to prior to SB 1. However, I demonstrate using Professor Mayer's data that there was also a large increase in ballot rejections that had nothing to do with the identification requirements of SB 1. This casts serious doubt on the credibility of the pre-SB 1 ballot rejection numbers, and of comparisons of rejection rates before and after SB 1.

80. Finally, Professor Mayer would have you believe that SB 1 had a disparate adverse impact against Black and Hispanic voters. Again, his evidence is unconvincing. Professor Mayer has no data on whether a voter is Black or Hispanic. Rather, he is using educated guesses based on surname and residential neighborhood. I demonstrate that the documented classification error inherent in these educated guesses makes it difficult to conclude anything with certainty regarding differences across actual race or ethnicity. Moreover, I demonstrate that Professor Mayer computed disparate impact of SB 1's mail-in voting safeguards incorrectly in Harris County and Dallas County. The correct approach results in the opposite conclusion of disparate impact against predicted-Whites

in favor of predicted-Hispanics, and no evidence at all of disparate impact against predicted-Asians. Similarly, I show there is no evidence of any adverse impact of the elimination of drive-thru voting in Calhoun County, either in the aggregate or for predicted-minorities. In addition, I show that implementing Professor Mayer's flawed statistical test in Calhoun County yields a conclusion of disparate impact on 2020 voters who did not even use the drive-thru voting option. This provides further evidence of the unreliability of Professor Mayer's approach to assessing disparate impact.

Respectfully Submitted,

Mark Hoekstra, PhD

CURRICULUM VITAE

Mark Hoekstra

Texas A&M University
Department of Economics
275 Liberal Arts Social Sciences Building
College Station, TX 77840-4228

Office Phone: 979.845.7302
Email: markhoekstra@tamu.edu
https://sites.google.com/view/markhoekstra
Google Scholar Profile (link)

Academic Appointments

2018 – Present	Professor of Economics
2015 – Present	Private Enterprise Research Center Rex B. Grey Professor of Economics, Texas A&M University
2011 – 2018	Associate Professor of Economics, Texas A&M University
2006 – 2011	Assistant Professor of Economics, University of Pittsburgh

Research Appointments

2015 – Present	Research Associate, National Bureau of Economic Research
2013 – Present	Research Fellow, IZA
2011 – 2015	Faculty Research Fellow, National Bureau of Economic Research

Editorial Positions

2018 – Present	Associate Editor, *Journal of Labor Economics*
2015 – Present	Associate Editor, *Journal of Human Resources*

Education

Ph.D. Economics, University of Florida, August 2006
Dissertation Advisor: David Figlio

B.A. Economics, Hope College (*summa cum laude)*, June 2001

Research Interests

Applied Microeconomics, including Labor Economics, Law and Economics, and the Economics of Education

Publications

"The Effect of Open-Air Waste Burning on Infant Health: Evidence from Government Failure in Lebanon" (with Pierre Mouganie and Ruba Ajeeb), forthcoming in *Journal of Human Resources*

"The Effect of School and Neighborhood Peers on Achievement, Misbehavior, and Adult Crime" (with Stephen B. Billings), forthcoming in *Journal of Labor Economics*

"Does Race Matter for Police Use of Force? Evidence from 911 Calls" (with CarlyWill Sloan), *American Economic Review* 2022, 112(3): 827-860.

"The Effect of Own-Gender Jurors on Conviction Rates" (with Brittany Street), *Journal of Law and Economics* 2021, 64(3): 513-537.

"(Almost) No One Votes Without ID, Even When They Can" (with Vijetha Koppa), *Economics Letters* 2021, 205: 1-3.

"The Impact of College Diversity on Behavior Toward Minorities" (with Scott E. Carrell and James West), *American Economic Journal: Economic Policy* 2019, 11(4): 159-182.

"The Long-Run Effects of Disruptive Peers" (with Elira Kuka and Scott E. Carrell), *American Economic Review* 2018, 108(11): 3377-3415.

"Peer Quality and the Academic Benefits to Attending Better Schools (with Pierre Mouganie and Yaojing Wang), *Journal of Labor Economics* 2018, 36(4): 841-884.

"Cash for Corollas: When Stimulus Reduces Spending" (with Steven L. Puller and Jeremy West), *American Economic Journal: Applied Economics* 2017, 9(3): 1 – 35.

"Illegal Immigration, State Law, and Deterrence" (with Sandra Orozco-Aleman), *American Economic Journal: Economic Policy* 2017, 9(2): 228-252.

"Vehicle Miles (Not) Traveled: Why Fuel Economy Requirements Don't Increase Household Driving" (with Jeremy West, Jonathan Meer, and Steven L. Puller), *Journal of Public Economics* 2017, 145: 65-81.

"Are School Counselors an Effective Education Input?" (with Scott E. Carrell), *Economics Letters* 2014, 125(1): 66-69.

"Bank Privatization, Finance, and Growth" (with Daniel Berkowitz and Koen Schoors), *Journal of Development Economics* 2014, 110: 93-106.

"Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine (with Cheng Cheng) *Journal of Human Resources* 2013, 48(3): 821-854.

"Family Business or Social Problem? The Cost of Unreported Domestic Violence" (with Scott E. Carrell) *Journal of Policy Analysis & Management* 2012, 31(4): 861-875.

"Is Poor Fitness Contagious? Evidence from Randomly Assigned Friends" (with Scott E. Carrell and James West) *Journal of Public Economics* 2011, 95(7-8): 657-663.

"The Ticket to Easy Street? The Financial Consequences of Winning the Lottery" (with Scott Hankins and Paige Marta Skiba) *Review of Economics and Statistics* 2011, 93(3): 961-969.

"Does Drinking Impair College Performance? Evidence from a Regression Discontinuity Approach" (with Scott E. Carrell and James West) *Journal of Public Economics* 2011, 95 (1-2): 54-62.

"Does High School Quality Matter? Evidence from Admissions Data" (with Daniel Berkowitz) *Economics of Education Review* 2011, 30(2): 280-288.

"Lucky in Life, Unlucky in Love? The Effect of Random Income Shocks on Marriage and Divorce" (with Scott Hankins) *Journal of Human Resources* 2011, 46(2): 403-426.

"Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids" (with Scott E. Carrell) *American Economic Journal: Applied Economics* 2010, 2(1): 211-228.

"The Effect of Attending the Flagship State University on Earnings: A Discontinuity-Based Approach" *Review of Economics and Statistics* 2009, 91(4): 717-724.

Other Publications

"Returns to Education Quality". 2020. In Steve Bradley and Colin Green (Eds.), *The Economics of Education: A Comprehensive Overview, 2nd edition*. Edited by Steve Bradley and Colin Green. Elsevier Academic Press.

"Domino Effect" (with Scott E. Carrell). 2009. *Education Next:* 9(3). Available at http://www.hoover.org/publications/ednext/Domino_Effect.html.

Working Papers

"The Scale and Nature of Neighborhood Effects on Children: Evidence from a Danish Social Housing Experiment" (with Stephen B. Billings and Gabriel Pons Rotger)

"Illegal Immigration: The Trump Effect" (with Sandra Orozco-Aleman)

"When Should We Trust Weighted Least Squares Estimates?" (with Cheng Cheng)

Awards

IZA Young Labor Economist Award, 2012 (with Scott E. Carrell)

Teaching Experience

Texas A&M University:
Sports Economics, Public Economics I (PhD-level), Econometrics II (1st-year PhD), Labor Economics I (2nd-year PhD)

University of Pittsburgh:
Labor Economics (PhD-level), Sports Economics, Intermediate Public Finance, Industrial Organization, and Research Methods in Empirical Microeconomics

University of Florida:
Public Finance and Managerial Economics

Department Service

Executive Committee (Fall 2011 – Fall 2014; Fall 2016 – Spring 2017)
Graduate Instruction Committee (Fall 2012 – Spring 2019)
Director of PhD Admissions (Fall 2012 – Spring 2015; Fall 2018 – Spring 2019; Spring 2023)
Director of PhD Program (Fall 2012 – Fall 2014)
Applied Microeconomics Search Committee (2011-12, 2012-13, 2014-15)

Primary Dissertation Advisor (Initial Placement, Current Position)
(Non-tenure track positions and co-advisor roles are noted if applicable; excludes committee memberships)

Suhyeon Oh	(expected 2025)
Maya Mikdash	(expected 2024)
Adam Bestenbostel	(2022, Air Force Academy, non-tenure-track Assistant Professor)
Meradee Tangvatchaparong	(2021, 5-year non-tenure-track Assistant Professor, Hitotsubashi University's Institute of Economic Research))
CarlyWill Sloan	(2020, Claremont Graduate University, now at United States Military Academy West Point)
Brittany Street	(2019, University of Missouri)
Abigail Peralta	(2018, Louisiana State University)
Yaojing Wang	(2017, Bank of America, co-advised with Li Gan, now at Peking University)
Vijetha Koppa	(2016, Stephen F. Austin State University, now at Institute of Management

	Technology, Dubai)
Jillian Carr	(2015, Purdue University)
Pierre Mouganie	(2015, American University of Beirut, now at Simon Fraser University)
Gonzalo Sanchez	(2015, Pontificia Universidad Católica de Ecuador)
Cheng Cheng	(2014, University of Mississippi, now at Amazon)

Presentations

Essen Health Conference (keynote speaker, scheduled May 2023); Clemson University (November 2022); Berlin Applied Micro Seminar, October 2022; Simon Fraser University, April 2022, Jinan University, October 2021; National University of Singapore, April 2021; University of Florida, April 2021; ASSA American Economic Association Annual Meeting (x2), January 2021; San Diego State University, October 2020; Boston University, September 2020; University of Maryland, September 2020; Notre Dame, September 2020; NBER Summer Institute – Crime, July 2020; Claremont McKenna College, February 2020; Claremont Graduate University, January 2020; American Economic Association Annual Conference, January 2020; Southern Economic Association Annual Conference, November 2019; Victoria University of Wellington Applied Econometrics Workshop, October 2019 (keynote speaker); University of Mississippi, October 2019; Mississippi State University, October 2019; Stata/Texas Applied Microeconomics Conference, October 2019; University of Florida, May 2019; Georgia Tech, March 2019; West Virginia University, March 2018; University of Tennessee, January 2018, Purdue University, January 2018; University of Kentucky, October 2017; Annual Meeting of the Western Economic Association, June 2017; University of Leicester, June 2017; University of Leicester Domestic Violence Workshop, June 2017; American University of Beirut, March 2017; University of Uppsala, March 2017; Montana State University, April 2016; American University of Beirut, March 2016; Columbia University, February 2016; Annual Meeting of the American Economic Association Meeting (January 2016); Annual Meeting of the Southern Economic Association (November 2015); NBER Education Program Meeting (November 2015); Brigham Young University, February, 2015; Federal Reserve Bank of New York, February, 2015; Stata/Texas Applied Microeconomics Conference, November 2014; University of Florida, November, 2014; Louisiana State University, October 2014; Institute for the Study of Labor (IZA), October 2014; University of Wisconsin-Milwaukee, October 2013; Ghent University, September 2013; University of Texas – Dallas, April 2013; Stata/Texas Applied Microeconomics Conference, December 2012; Southern Economic Association Annual Meeting, November 2012; University of Texas-Austin, April 2012; Georgetown Public Policy Institute, April 2012; University of Missouri, October 2011; Baylor University, August 2011; Texas A&M University, November 2010; University of Houston, October 2010; University of Pittsburgh School of Medicine, Psychiatry and Epidemiology Seminar, October 2009; NBER Summer Institute, Law and Economics Program, July 2009; University of California at Davis, April 2009; University of California at Berkeley Labor Lunch, March 2009; American Economic Association Annual Meetings, January 2009; Texas A&M University, September 2008; Carnegie Mellon University, September 2008; NBER Summer Institute, Economics of Education Program, July 2008; Society of Labor Economists Annual Meeting, May 2008; Vanderbilt University, April 2008; NBER Education Working Group, November 2006

Other Information

Referee: *American Economic Journal: Applied Economics, American Economic Journal: Economic Policy, American Economic Review, American Journal of Health Economics, American Sociological Review, Berkeley Electronic Press, Contemporary Economic Policy, Economic Development and Cultural Change, Economic Inquiry, Economic Journal, Economics of Transition, Education Economics, Education Finance and Policy, Empirical Economics, European Journal of Law & Economics; Journal of Applied Econometrics, Journal of Comparative Economics, Journal of Demographic Economics, Journal of the European Economic Association, Journal of Health Economics, Journal of Human Resources, Journal of Labor Economics, Journal of Policy Analysis and Management, Journal of Political Economy, Journal of Population Economics, Journal of Public Economics, Journal of Sports Economics, Journal of Urban Economics, Labour Economics, Proceedings of the National Academy of Sciences (PNAS), Quantitative Finance, Quarterly Journal of Economics, Regional Science and Urban Economics, Review of Economics and the Household, Review of Economics and Statistics, and Southern Economic Journal.*

Reviewer: Israel Science Foundation, National Science Foundation, Marsden Fund (New Zealand), Dutch Research Council

Citizenship: United States